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Interactive comment on “Monitoring Atlantic overturning circulation variability with GRACE-type ocean bottom pressure observations – a sensitivity study” *by* K. Bentel et al.

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Statement of Revision

We greatly thank both reviewers for their valuable comments which helped to improve our manuscript significantly. In the following we want to give our statement to Referee #1's comments.

Comments by Anonymous Referee # 1

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General comments:

The theory presented in the article is neither novel nor very complicated (which could justify discussing theoretical and real results in separate papers), the framework to derive the AMOC variations from GRACE-like data has been set up and the GRACE data performing best (JPL CRI mascons) are available at the author?s institute. Therefore, including actual results should be feasible (although not trivial, given that the noise in the GRACE data was neglected in the sensitivity study). This would make for a much more exciting paper, with a much higher contribution to the scientific progress. However, as said before, the methods and results look sound, so I will leave it up to the editor to decide if this manuscript should be accepted for publication in its present form.

The Referee states that the article might lack of scientific results since the article does not show any results from real data. We share the opinion of Referee #2, who states that the article would result in an extremely long article if results from real data were to be included. Indeed, the basic theory behind deriving transport from ocean bottom pressure is not very complicated. However, using ocean bottom pressure from satellite gravimetry to derive ocean transports is not trivial, since the signals are on the edge of the resolution and sensitivity of satellite gravimetry. We believe this simulation study is essential and justified to examine sensitivity of the available ocean bottom pressure observations derived from satellite gravimetry. It is important to know about strength and weaknesses of the method before simply applying it to real data. Furthermore, there are not many appropriate validation data sets for results from real data. In-situ data are sparse, and they are point measurements, while satellite observations cover large areas. Furthermore, in-situ data also have errors and suffer from drift. Assessing transport from other remote sensing methods, like altimetry, have errors that propagate into a transport estimate.

Thus, we believe the present study serves as a rigorous assessment of what to expect from the data with regards to recovering the MOC signal. It will serve as a guide for the user to better understand the advantages and limitations as well as provide an

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error estimate of the impacts of the resolution.

The authors find that GRACE-like observations can capture AMOC variations with an interannual RMS error of about 1 Sv. However, it's unclear how this compares to the interannual variability of the AMOC itself. Is this 1 Sv error small enough to still detect a useful signal? This deserves to be discussed in more detail.
We added a new Fig. 9 in order to clarify this point and discussed in in text. The error of about 1 Sv is about 50& of the signal magnitude of AMOC anomalies, which is roughly about 2 Sv. Fig. 9 shows that GRACE-like OBP observations are nevertheless capable to capture the signal with a correlation of 0.63 with the model reference (for this example-latitude).

Also, the error strongly depends on how one corrects for hydrological leakage. The CRI mascons and optimally placed mascons perform best in most case. In this regard, it should be kept in mind (and mentioned in the manuscript) that the spherical harmonic solutions can be corrected for leakage as well using stand-alone hydrology models (albeit only to a certain extent, since these models aren't perfect). Furthermore, I suggest to include an additional column of figures in figure 7, showing RMS errors for the GRACE simulations without any hydrology included. This way the reader gets a better feel for which part of the error is caused by hydrological leakage, and which part by leakage due to steep bathymetry gradients.
Indeed, spherical harmonics solutions can be corrected for leakage as well. But, as the Referee mentions, these models have errors, and together with the required smoothing in the spherical harmonics (300-500 km Gaussian filter, in order to reduce the north-south stripes), the leakage correction does not sufficiently reduce the overall errors in the already small ocean bottom pressure signal, since other errors are introduced and smeared out again with the smoothing. We added a short paragraph to section 2.4.1 in order to clarify this.

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The suggested additional column has been added to Fig. 8 (instead of Fig. 7). It shows now time series with and without hydrology in a direct comparison. We believe that this provides the reader a better feel for the effect of continental hydrology, as mentioned by the Referee.

The simulations and results are based on the assumption that the GRACE data are free of error (BTW, this should be mentioned clearly in the abstract). This is justified, but a short discussion should be included on how these errors will affect the results when working with real data. Chambers and Bonin (OS, 2012) found an error of 1.5-2.5 cm water equivalent in the North-Atlantic. How would this translate into AMOC transport error?

We included this in the abstract and in section 2.3.

The most prominent errors in the GRACE solution are the correlated errors, resulting in North-South stripes in the spherical harmonic solutions. A lot of smoothing is required to reduce these errors. We applied a Gaussian filter with a 300 km radius to smooth the data, corresponding to the minimum amount of smoothing which would be required for real data. Additionally, we chose a spherical harmonic expansion up to d/o 60, which approximately corresponds to the d/o up to which signal content would prevail over the noise content in real data. The mascon solutions have intrinsically removed much of the correlated striping errors through the application of geophysical constraints, and no additional smoothing is required (the mascon-smoothing is effectively mimicked by the 3-degree binning). That means we can assume that no additional smoothing is required for the mason solutions. In this application here, the limitation is not predominantly the actual error on the GRACE gravity data, but first and foremost the spatial resolution, which is closely related to assigning the appropriate depth to each OBP observation. The error caused by assigning the OBP value to the incorrect depth is still by far the dominant error source here and the limitation to using GRACE data to derive ocean transport. Therefore, focus is more on the leakage effects (i.e. assigning the correct depth to the OBP observations) than actual GRACE errors.

Chambers and Bonin (OS, 2012) found a mean error of 2 cm in the spherical harmonic GRACE solutions in the North Atlantic. A 2 cm error in ocean bottom pressure would result in an error of about 0.002 Sv/m in the derived transport. Assuming the northward transport layer spans roughly 1000 m, this leads to an error of about 2 Sv from spherical harmonics. However, we note that mascon data errors are estimated to be about 30% smaller than this in the current study region (see Watkins et al. 2015).

Finally, an important scientific question is whether the AMOC is declining in strength or not. The summary and outlook sections should briefly mention this and discuss how feasible this is with GRACE data (will GIA be a problem? How many years of observations would be required to detect a significant trend, given the 1 Sv error in this study?).

Indeed, AMOC trends are of great interest. Trends in the ocean bottom pressure can be recovered by GRACE, however, the uncertainty in GRACE trend corrections for GIA (from models) and leakage correction can significantly corrupt a transport-related OBP trend. In addition, the ECCO2 model is not free from model drift, which must be corrected for. An unrealistic drift could contaminate the statistics here. Therefore, we decided to remove the trend and only focus on interannual signals. A detailed trend correction study for GRACE is beyond the scope of this paper, however, we intent to pursue a detailed evaluation in future studies (same answer as to Referee #2's question).

-minor comments:

* p 1771, line 6: Why are you using a 15-month running mean and not, for example, 13 (1 yr) months or 19 months (1 1/2 yr). Add motivation.

A 15 months running mean is chosen in order to obtain interannual signals only. The exact filter length does not affect the results in any significant way, any fiter length from 13 to 19 months could have been used.

* p 1771, line 8-21: Many of the readers of Ocean Science are not familiar with the GRACE data and the different products available. I think it would be good to give a short description of the standard products (spherical harmonics) and the mascon products, and how they differ in use and spatial/temporal resolution.
A short discussion about the available GRACE products is added to section 2.3.

* p. 1772, line 3: 'Aliasing' might not be the best choice here, suggest to change to "contamination" (in the GRACE jargon, 'aliasing' usually refers to high-frequency signals causing spurious long-term signals due to the temporal sampling by the satellites).
Changed.

* section 2.4: The SH60 data is listed as having a spatial resolution of 3 degrees in table 1, but looking at the detail in figure 3c this appears incorrect. How did you define your grid for the spherical harmonics solutions: a regular lat/lon grid, or did you shift the grid in longitudinal direction (for each latitude) so that the grid points are optimally placed along the coast line? Just as for the mascons solutions, such a variable grid will most likely reduce leakage from hydrology and improve the results.
All the grids, on which ocean bottom pressure data is sampled, are regular 0.25 deg lat-lon grids. However, this grid sampling does not define the resolution of the data. It accommodates the highest resolution, which is the original 0.25 deg ECCO2 data. However, the smoothed data is still sampled on the same grid. Therefore, grid sampling or location of the grid cells does not affect the coastal resolution of the spherical harmonic data. The resolution is restricted by the maximum spherical harmonic degree and order (which is 60, and corresponds to about 3 deg). In contrast to the spherical harmonic truncation, the mascon data is binned to 3 degree mascon cells, and the location of the cells actually makes a difference.

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* Section 3.1: The RMS errors of the mascon solutions show a peculiar feature at 29-30 N, extending to about 3000 m depth, which is absent in the spherical harmonics solutions. Any idea what's causing this? Please discuss briefly in the manuscript. Also, would it be possible ot apply the CRI approach to the position optimized mascons to further reduce the RMS error?
At about 25-30 N the topography on the Western boundary is extremely steep, in particular between 1000 and 3000 m depth (In Fig. 2 the 1000 and 3000 m depth contour lines are extremely close together). This steep gradient causes a lot of leakage in one interpolated mascon cell, which covers depth layers from above 1000 m to below 3000 m, but gets assigned a single averaged OBP value. The SH solution does appear to perform somewhat better here, but the actual measurement errors likely would cancel this apparent advantage. We discuss this briefly in section 3.1 now. Regarding the combination of CRI and position optimized mascons please refer to our response to the first comment of Referee #2.

* Section 3.2, first paragraph & figure 6 + 7: Since GRACE cannot observe the mean AMOC, but only its variations. You also should indicate where the maximum (interannual) variabilty occurs in the model domain. In figure 7, you should also include a line showing the RMS of the AMOC transport at the three layers so the reader can get a feel of the signal-to-noise ratio. How the RMS errors of the GRACE simulations compare to the RMS of the model AMOC should also be discussed in the main text, conclusions and abstract.
A new panel is added to Fig. 6 to show the variability of the transport signal (derived from ECCO2). Figure 7 is updated and described in the text as recommended by the Referee.

- Tables & Figures:

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* Figure 1: I found figure 1 rather confusing and not very illustrative. OBP +/- signs are plotted at intermediate depths, although these are only observable at the bottom (as the name suggests). Furthermore, I'm struggling to understand what the mean and anomalous flow exactly refer to and what the dashed line is supposed to identify. I suggest to re-do this plot and show the mean northward flow from the ECCO2 model, with OBP plotted separately on the X-axis.
We notice that due to the steep slopes in the more realistic topographic cross section caused the confusion on whether the +/- signs indicated OBP at intermediate depths. This is, of course, not the case so we changed the figure to a more generic illustration (not including real topographic data).

* Figure 7: a line indicating the required level of correlation for significance should be included in the correlation plots.
Adding another line makes the plots crowded and less readable. We found it more helpful to indicate the significance level in the caption.

- Technical comments:
* Define the GRACE acronym on first occurrence
* p. 1767, line 12: to monitor *the* AMOC.
* p. 1772, line 7: CRI, define abbreviation on first occurrence.
* p. 1773, line 4: change "60°" to "degree and order 60"
* formula 1: define 'eta' symbol
* p. 1770, lines 6-13: this paragraph is a partial repeat of lines 19-25 on page 1767. Restructure these two paragraphs to avoid overlap.
All changed

Interactive comment on Ocean Sci. Discuss., 12, 1765, 2015.